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THE INTERIOR OF THE EARTH.

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IT IS a matter for surprise that the alarmists who from time to time predict the impending destruction of the world pay so little attention to the opportunities which geology offers them. By making two or three assumptions, each largely accepted, such a prophet could work out a scene of horror well calculated to make the stoutest heart grow faint.

A very large part of the educated public believes that the earth is a molten globe superficially enveloped by a chilled crust, and a magazine article in support of such a theory has recently attracted much attention. Very many of the natural philosophers consider it most probable that the rocks at and near the surface of the globe would expand in melting. If the earth were thus constituted a time would come when the solid crust would crack from its own weight, or from some moderate internal disturbance; and then block after block of the crust, region after region of the world we know and love so well, would plunge slowly and heavily to meet the rising, molten flood, while whirlwinds of scalding steam would shroud perishing humanity.

It would require a Dante to do justice to the tragic side of this theme. A hard-hearted physicist would simply remark that a crust of such dimensions resting on a fluid of inferior density is in unstable equilibrium; the rest being an evident consequence. Statements such as this are commonly considered as extremely uninteresting; but the Dantesque view of the subject has been indicated sufficiently to show that the earth's interior is within the sphere of human interest. Aside from ignoble fears, there seems scarcely any topic better suited to excite a legitimate intel-

lectual interest among men than this most fundamental question concerning that little planet, our world. Is it a molten globe with a pellicle of cool dry land, or is it really *terra firma,* a solid earth ?

It would be interesting to trace the rise and progress of the two existing and opposed opinions on this question, but economy of space forbids more than the barest outline of such a sketch. The Greeks and Romans recognized the existence of melted lavas beneath the surface, though not supposing the whole interior of the earth fluid. Descartes, in 1644, seems to have been the first to give expression to the opinion, still so widely entertained, that the earth is a fiery globe, superficially encrusted. Newton and Laplace on the other hand assumed that the earth was solid ; and, indeed, incapable of deformation by the attraction of the moon or sun.

Laplace and his successors were not led by their own investigations to abandon the idea of a solid earth as inconsistent with known phenomena, for their study of the tides on an ideally rigid globe have been in great measure verified by observation. Indeed, when the irregularities of the sea bottom and of the sea shores are taken into consideration, it seems strange that human ingenuity could have advanced so far towards a satisfactory accounting for the facts. Evidently, then, these results were sufficient justification for the assumption that the earth behaved like a mass approaching ideal rigidity, or presenting great resistance to deformation. On the other hand geologists in Laplace's day were substantially unanimous in regarding the earth from Descartes' point of view as an incrusted globe, the interior of which was liquid. There are indeed arguments for this opinion which are not lightly to be disposed of, and which could not have been fully answered early in the century. It will be well to state these reasons first and then to examine them.

One of them rests upon the observed fact that, in mines and other artificial excavations, the temperature of the rocks increases as one descends from the surface, the increase being about one degree Fahrenheit for every sixty-four feet, or more than eighty-two degrees per mile. Hence an enormous temperature must exist at depths of twenty or thirty miles, one which would probably suffice to melt most rocks existing at the surface ; at least under those conditions with which we are familiar. A condition

with which we are not at all familiar is the immense pressure existing at such depths. Again, the eruption of lavas, which probably first led to the idea of internal heat, seems an argument for the existence of reservoirs of melted rock, if not for general subterranean fluidity. Strongest of all in the minds of many geologists is the argument derived from the crumpling and rupture of rocks. It is absolutely certain that areas of sediment, which were laid down in seas or lakes as substantially horizontal layers of mud or sand, have not only hardened to firm rock but have been bent, twisted and folded to an extraordinary extent. They are sometimes as minutely and as sharply flexed as a crumpled pocket handkerchief. If the superficial portion of the earth is a crust floating on a cooling shrinking fluid, it might perhaps pucker like the skin of a drying orange ; and indeed mountain ranges are no larger in proportion to the earth than the tiny folds of a shrivelled orange skin are in proportion to the orange.

None of these argumeuts is wholly conclusive. There are certain substances, of which ice and cast-iron are the most important, which contract in melting; and pressure (within certain limits) facilitates their fusion because it coöporates with heat to reduce the volume of the mass. This class is a limited one. All other substances expand in fusing, and pressure, by opposing this expansion, impedes their fusion. Now it has long been known from experiments that rocks may be fused to glassy slags, and that these slags when cold are much less dense than the rocks from which they are derived. Glasses and glassy slags themselves also expand in melting, or shrink in solidifying. It is for this reason that moulded glass utensils have rounded edges and cannot be made closely to imitate cut-glass. Thus cold rocks are denser than the melted glasses into which heat converts them.*

More satisfactory in some respects than any experiments are observations which I have made in the Yosemite Valley and other deep cañons of the Sierra Nevada. In that region there are

* Very numerous experiments have been made on the change in density in rocks and minerals due to fusion and, indeed, by investigators of great distinction. Among them are C. Sainte-Claire Deville, Thoulet, Rammelsberg, Abich and Magnus. Many of the data are collected by Professor Justus Roth in his Allgemeine Geologie, vol. 2, 1883, page 51. They all show a diminution of density by fusion, for the most part of very large amount. Thus granite and allied rocks increase in bulk by about ten per cent. when fused.

Certain anomalous results obtained by Mr. Joseph Whitley, of Leeds, for granite and whinstone cannot outweigh scores of experiments by the ablest investigators.

many narrow dikes of a light colored granite which must have cooled under a "head" of several thousand feet of melted rock. Under such pressure the liquid rock must have filled the cracks it occupied very completely. In hundreds of cases, however, flat, lenticular openings have formed along the centres of these dikes while the rock was solidifying and crystallizing. This is a conconclusive proof that the material shrank, or that the volume of the rock diminished, in passing from the fluid to the solid state.

It is substantially certain from such facts that pressure would impede the fusion of rocks by counteracting the expansive tendency which accompanies fusion. Consequently a given rock at a depth of several miles below the surface would melt only at a higher temperature than would suffice to fuse it at the surface.

More precise information as to the relations between melting point and pressure was highly desirable and indeed constituted one of the great desiderata of physical geology. This has lately been obtained by Dr. Carl Barus, at the instigation of Mr. Clarence King, for a very typical rock.* This is the material of the Palisades on the Hudson; it covers large areas in northern New Jersey, and is in fact very abundant throughout the world. It is called diabase and sometimes basalt. Dr. Barus showed that this rock expands in melting, that the temperature at which it melts increases with the pressure, and that the increase in this temperature is simply proportional to the increase in pressure. He found also that various other substances, such as wax and paraffine, behave in substantially the same way as diabase so far as the relation of melting point to pressure is concerned; and there is practically no doubt that granitic rocks would show the same relation, since it is known that they yield a glass specifically lighter than the unfused rock, just as diabase does.

Now unless the temperature increases in passing from the surface of the earth towards its centre so fast as to overtake the melting point of the rock (which rises with pressure) no portion of the earth can be fiuid. Just how fast the temperature rises at considerable distances cannot, however, be determined directly; so that the fine work mentioned in the last paragraph, taken by itself, only shows that the distribution of temperature in the earth *may* be such as to produce no general fusion of the rocks. Thus it does not follow that because the temperature rises with

* *Am. Jour. Sci.*, vol. 43, 1892, p. 56, and vol. 45, 1893, p. 1.

increasing depth from the surface, the interior *must* be fluid, and one of the supposed grounds for concluding that the inner earth is a molten mass is untenable.

As for the evidence of volcanoes, they do not even tend to show a general fluidity of the interior, any more than springs of water in the Alps prove that the interior of that range is a subterranean lake. Volcanoes do not even show that there are permanent reservoirs of melted matter beneath the surface. If the rocks at a few miles below the surface are kept from melting only by the pressure, a relief of pressure would cause them to melt. Similarly when a boiler bursts, the water being above the ordinary boiling point flashes instantly into steam. Consequently volcanic eruptions may possibly be due to relief of pressure by cracks from the surface, and no permanent reservoirs of melted lava are then requisite. Or, again, the melting may be induced in the highly heated solid rock by a small accession of heat due to local mechanical action, such as "faulting." A third possibility is of a chemical nature. All melted lavas contain water, and are to some extent in a condition of so-called aquo-igneous fusion. This fusion in the presence of water (under a pressure which prevents the escape of steam) takes place at a lower temperature than dry fusion and, within certain limits, the more water the mass contains the more easily it will melt. Hence water, percolating through cracks or even pores of the rock to great depths, may sometimes so reduce the point of fusion of the material in this way that it will pass into the liquid state without either relief from pressure or accession of temperature. This hypothesis would contribute to an explanation of the eruptions of the Hawaiian volcanos where there is nothing to indicate the presence of deep cracks or active faulting. On the whole, then, volcanoes do not imply the presence even of permanent reservoirs of fluid lava, much less the fluidity of the whole interior of the earth.

As for the deformation and wrinkling of the rocks near the earth's surface, it is a subject upon which much light has been thrown since the middle of the century. When one bends a bit of lead, copper, or tallow the particles of the mass slide past one another, very much as the leaves of a book do when it is opened or shut, and remain nearly in the position into which they are forced. This relative inelastic movement among the particles is called the "flow of solids" by Tresca, and such flow takes place

whenever a mass stays bent or otherwise deformed after the force is relieved. In ordinary experience only soft solids, such as those mentioned, flow sensibly. But under slowly applied forces, gradually increasing in intensity, even glass and hard steel flow, as experiment proves.

Rock exposures often show in an unmistakable manner the phenomena of solid flow. In particular, many conglomerates, or pudding stones, are found to have been moulded by pressure, something as if they had been reduced to a pasty condition; yet not really thus, for the details of structure prove that the pebbles have obeyed the law of solid flow, not those of liquid flow. The distinction is most marked, but it cannot be explained here for lack of space.*

If, then, the rocks yield, in large measure at least, by solid flow (pushed to an extent which the feeble resources of experiment cannot reproduce), it is quite unnecessary to assume that the earth as a whole has received its superficial plications by a different process. If the external shell of the earth, say ten or twenty miles in thickness, is crumpled by processes identical with those which twist and bend the steel beams of a collapsing railway bridge or of a falling building, a thicker shell, perhaps the outer one or two hundred miles of the globe, would yield in the same way, only more completely because of its greater weight. In other words, the contortion of the superficial rocks does not necessarily imply any fluid interior or substratum.

Thus none of the reasoning from which a fluid interior has been inferred is convincing; and indeed it may be concluded that simple examination of the earth's surface, however careful, is essentially inadequate to solve the question at issue.

"Papa," one may fancy Miss Edgeworth's Frank inquiring, "is this a solid rubber ball or only a hollow one filled with some liquid?" That idealized parent would not have replied: "Let me get out my microscope and carefully examine the surface of your toy." No, indeed! He would have answered: "Pinch it, my son, and judge for yourself."

Though the earth is too large for the immediate application of this method, the sun and the moon do really drag the earth out of shape with a tremendous energy adequate to the occasion. Do they deform the globe as if it were solid, or as if it were filled

* Bulletin Geological Soc. of Amer., vol. 4, 1893, p. 46.

with fluid? This is the best test of the question which can possibly be suggested. It was not an easy matter to substitute computation for the muscular sense which would have led Frank to a correct conclusion. The earth retains pretty nearly a constant shape in part by the attraction of gravity. If it is a solid mass its elasticity also opposes the deforming action of the sun and moon. Laplace showed how the earth would behave if it resisted deformation only by gravity. Lamé showed how a supposed solid sphere would resist deformation if only its elasticity came into play. Lord Kelvin (then Sir William Thomson) combined these results and compared them with observations on the actual deformation.* The problem as thus solved is one of great mathematical complexity, but I have shown that Lamé's results can be dispensed with for the purposes of this inquiry, and that the solution can be reached without loss of stringency by a very simple method.† The conclusion, however reached, is this:

If the earth were fluid and had a very thin crust the earth as a whole would be drawn towards the moon and sun substantially as the water of the ocean is attracted; so that there would be a "bodily" tide in the earth as a whole twice a day; and this tide would be about as great as the oceanic tides are. Thus the coasts and the ocean would rise and fall together. What we recognize as a tide is simply the difference between the rise of the sea and the rise of the land. If the crust were thin this difference would be insensible or, in other words, we should not perceive the tides at the seashore any more than we perceive them from the deck of a vessel at sea. Shore and sea would rise and fall together.

Thus the mere fact that there are tides shows that the earth offers effective resistance to deformation. The next question is how great is this resistance. Lord Kelvin, and afterwards Professor G. H. Darwin, have computed how the resistance displayed by the earth to tidal deformation compares with that which a globe as elastic as solid, continuous glass would present. They find that a glass globe would certainly yield more freely than the earth. They have also compared it with a globe of steel, and they conclude that the earth resists the attraction of the sun and moon nearly or quite as much as a solid continuous globe of steel would do.

Now it is hard to imagine that the earth is as strong as steel,

* See Thomson and Tait, Natural Philosophy, section 834.
†*Am. Jour. Sci.*, vol. 39, 1890, p. 336.

still harder to fancy that it is stronger. But if the world consists of even a very thick shell (say five hundred miles in thickness) inclosing a fluid mass, this shell must be far stronger than steel or any other substance known to men. Hence, finally, it is substantially certain that the earth is solid to (or close to) the centre.*

Some geologists have demurred to this conclusion, for it is hard to give up opinions which have been considered certain and fundamental; but only one serious attempt has been made to account for the acknowledged fact of the enormous effective resistance of the earth to the tide-producing forces consistently with a fluid interior. This attempt is due to Reverend Osmond Fisher,† and his speculation has recently been restated by the famous zoölogist, Mr. Alfred Wallace, in the *Fortnightly Review* for November. One may consider the attraction of the moon as tending to produce two distinct changes in the earth; one a change of shape, the other a change of bulk or volume. Now Mr. Fisher supposed (quite erroneously) that Lord Kelvin had omitted to consider the earth as capable of changes of volume, and he maintained that the effect of this capacity would be to counteract the tendency to change of shape. This is simply a mistake. It can be and it has been shown, on unquestionable mechanical principles, that volume change would diminish the total effective resistance of the earth to tidal force and reduce the apparent height of the tides.††

A solid rubber ball, though very elastic, is nearly incapable of change of volume; but there is a kind of spongy rubber, used for erasing drawings, which is full of small vesicles or "air holes," and a ball of this material can easily be reduced in bulk. Now Mr. Fisher's proposition is equivalent to this: If one were to make a ball of spongy rubber containing the right proportion of vesicles, the ball would be as rigid as if it were made of steel. So stated Mr. Fisher's hypothesis is incredible, nor has it been accepted by competent judges.

Lord Kelvin's argument has not been weakened by any objection yet advanced against it, while the theory has received support from Professor Darwin's discussion of tides, and still

* W. Hopkins' argument for solidity from precession has been shown to be untenable. An account of it is omitted to save space.

† Physics of the Earth's Crust, second edition, 1889.

†† *Amer. Jour. Sci.*, vol. 39, 1890, p. 336.

more strikingly from results recently announced by Professor Simon Newcomb.

It is known to all readers of newspapers that, in recent years, some small varying changes in latitude have been detected at astronomical observations, and Mr. S. C. Chandler has discovered that these changes are periodic, so that the telescopes come back to the same latitude after a certain period. From a great number of observations in the two Americas, England and continental Europe, Newcomb finds this period four hundred and thirty days. The cause is probably unusual accumulations of ice and snow near, but not at, one or other of the poles. A temporary additional load of snow confined, for example, to Greenland would very slightly change the axis about which the earth would rotate. Now if the earth were ideally rigid (as Laplace considered it in his theory of tides) this new axis, as has long been known, would shift its position along a conical surface described about the old axis, and this progressive change of the axis of rotation would cause a change of latitude such as that detected, but with a period of only three hundred and six days. Newcomb finds that if the earth is not ideally rigid the length of this period must be increased, so that if the earth were deprived of all rigidity, or were fluid throughout, the length of the period would be infinite. Making certain probable assumptions as to the behavior of the ocean, and supposing that the earth as a whole resists deformation as much as steel, he finds that the length of the period would increase to four hundred and forty-one days, or only eleven days more than the observed period. Thus if the assumptions as to the behavior of the ocean are exact, the effective resistance of the earth is a little greater than that of steel, and in any case the earth presents just about that degree of resistance.*

The investigation of Messrs. King and Barus mentioned above revitalizes an argument for the solidity of the earth, put forward long ago by Lord Kelvin, and referred to at the beginning of this paper. Assuming that rocks would expand in melting, he pointed out that the crusts formed on the surface would break up and sink in the molten mass. Subsequently, because of some very inadequate experiments which were interpreted as indicating that rocks contract in melting, he withdrew this ar-

* Newcomb's investigations are set forth in two distinct papers, *Monthly Notices Royal Astronomical Society*, vol. lii., 1892, p. 336, and *Astronomische Nachrichten*, vol. cxxx., 1892, p. 1.

gument. The careful work of the American investigators shows that Lord Kelvin's original assumption and conclusion were entirely correct.*

In conclusion, then, all the arguments which have not been shown to be inconclusive or false indicate that the earth presents a resistance to deformation about as great as if it were a solid steel ball, aud that it actually is solid to, or nearly to, the centre. The permanent deformations to which it has been subjected near the surface are enormous, and their amount is seldom appreciated by astronomers or physicists ; but these deformations have been produced for the most part by the "flow of solids," and there is no known incompatibility between such distortions and the theory of a solid earth. The public may accept the theory of *Terra Firma* in peace, and those geologists who attempt to combat it can scarcely fail to lose their labor.

GEORGE F. BECKER.

* The conclusions in this paragraph are entirely inconsistent with the hypothesis that the earth's interior resembles very stiff sealing wax or asphalt substances which are known as "ultra-viscous fluids." This idea must also be rejected on other grounds, an explanation of which is not needful here.